Response to comments from Anonymous Referee #2

We would like to thank the reviewer for the constructive suggestions, which helped us improve the clarity of the paper. Based on the comments and suggestions, we have revised the manuscript.
Please find below our point-by-point responses (in red) to the reviewer.
Thank you again for your time in assessing our work.

Line 46: Can the authors clarify the sentence starting with "Therefore, air injected into the lower stratosphere…."? Based on the writing I am not sure how the previous sentences result in this statement. Also, "than into the troposphere" should be "than in the troposphere".
"In the tropics, the transport by the Brewer-Dobson circulation is upward. Therefore, air injected into the lower stratosphere has a longer residence time than into the troposphere and reaches further distances into the stratosphere."

For clarification, the sentence is reworded and replaced by the following:

"In the upwelling branch of the Brewer-Dobson circulation (BDC), the water vapor injected in the tropical lower stratosphere is transported meridionally to the temperate stratosphere. Due to the slow nature of BDC, this process results in a longer residence time of the injected stratospheric water vapor."

Line 48: is there a reference to explain the "tropically averaged level of all-sky zero net radiative heating"?

Added the following references for "zero net radiative heating" :

(Gettelman and Forster, 2002; Sherwood and Dessler, 2001,2002).

Gettelman, A., and P. M. de F. Forster, 2002: A climatology of the tropical tropopause layer. J. Meteor. Soc. Japan, 80, 911–924.
Sherwood, S. C., and A. E. Dessler, 2000: On the control of stratospheric humidity, Geophys. Res. Lett., 27, 2513 – 2516.
Sherwood, S. C., and A. E. Dessler, 2001: A model for transport across the tropical tropopause, J. Atmos. Sci., 58, 765 – 779.

Line 61: please clarify "cold trap". Is this the CPT? Also, delete "On the other hand" at the beginning of the sentence.

"On the other hand" → has been deleted

The cold trap is the coldest region in the tropical tropopause layer (TTL) that modulates the water vapor entering the lower stratosphere via cirrus formation (Jensen et al., 1996) and ice crystals sedimentation (Holton and Gettelman 2001). The corresponding low water vapor concentration and freeze-dried air is indicative of the dehydration process (Ryu and Lee, 2010).

Such freeze drying would appear to require slow upwelling in that region to produce cooling and condensation and to allow time for the ice crystals formed in the freezing process to sediment out.

The following details about the cold trap has been added to the manuscript.

"…Ice particles injected into the lower stratosphere bypass the cold trap which is the coldest region in the TTL that modulates the water vapor entering the lower stratosphere via cirrus formation (Jensen et al., 1996) and ice crystals sedimentation (Holton and Gettelman 2001), sublimate above the level of CPT, and contribute to stratospheric hydration.."

Jensen, E. J., O. B. Toon, H. B. Selkirk, J. D. Spinhirne, and M. R. Schoeberl, On the formation and persistence of subvisible cirrus clouds near the tropical tropopause, J. Geophys. Res., 101, 21,361- 21,375, 1996.

Ryu, J., & Lee, S. (2010). Effect of Tropical Waves on the Tropical Tropopause Transition Layer Upwelling, Journal of the Atmospheric Sciences, 67(10), 3130-3148.

Line 63: This should be the start of a new paragraph. I can't tell if that is the authors' intention in this template. If so, then ignore this comment.

Corrected

Line 66: the authors could expand this to include other estimates. Satellite based estimates by Sassen and L'Ecuyer place cirrus coverage much lower than what is quoted here.

To add the suggested references, the following details have been added to the manuscript:
"Spaceborne active remote sensing (i.e., CloudSat and Calipso) reported 35% of cirrus coverage within ±15° latitude and 56% within ±30° latitude of the equator (*Sassen et al.*, 2008, 2009). According Baran et al (2012), satellite measurements in the tropics revealed that cirrus cloud cover can be 60–80% at any given time."

Sassen, K., Z. Wang, and D. Liu (2008), Global distribution of cirrus clouds from CloudSat/Cloud-Aerosol Lidar and Infrared Pathfinder Satellite Observations (CALIPSO) measurements, J. Geophys. Res., 113, D00A12, doi:10.1029/2008JD009972.

Sassen, K., Z. Wang, and D. Liu (2009), Cirrus clouds and deep convection in the tropics: Insights from CALIPSO and CloudSat, J. Geophys. Res., 114, D00H06, doi:10.1029/2009JD011916.

Line 68: GCMs acronym is never defined

GCMs acronym is defined: (General Circulation Models)

Line 130: "(Fig. 1)" should refer to Fig. 3 instead.

Corrected: "Fig. 1" → "Fig. 3"

Line 169: delete "problematic" and describe this more clearly why artificial conversion rates are unphysical

", which is inherently problematic" → removed

The artificial conversion between particle types is not physical because it is arbitrary and relies on ad-hoc tunable parameters that cannot be directly measured. It leads to systematic bias of the model output.

Added → : " which relies on ad-hoc tunable parameters that cannot be directly measured".
Also, more details are stated in line 176

"Artificial transitions between predefined ice categories and the corresponding ad-hoc conversion parameters can systematically bias the model output (Morrison and Grabowski, 2008; Lin and Colle, 2009)."

Line 175: why do the m-D and vt-D relationships not properly capture the transitions in ice particle mass and fall speed during riming?

The explanation is in the following sentences.

Line 181 :"With the use of m–D and vt–D relationships, it is assumed that all modeled ice particles evolve and sediment along the same trajectory in mass–size and fall speed-size spaces."

Line 191:
"Light riming can expand the minor axis of ice crystals or fill in gaps in their shapes, leading to increased density and fall speed without affecting maximum dimension."

Light riming extends the minor axis of ice particles or fills in gaps to increase the effective density of ice particles, resulting in an increase of fall speed without an increase in maximum dimension. This process contrasts with the m–D relationships for rime ice, which relate increases in mass and fall speed to increases in maximum dimension (Jensen et al., 2017). The shapes and fall speeds of ice particles are assumed a priori.

Line 198: please explain what is meant by the 99th percentile and how this is calculated. Also, can the authors explain why they chose the 99th percentile? Why not the 75th percentile?

We consider 99th percentiles (top 1%) of Ice water content to mainly reflect high ice water content (HIWC). The 75th percentile is not representative for this extreme value.

Line 202: add “value” to the end of the sentence

Added

Line 205: The statement beginning with “to examine the sensitivity…” includes a lot of repeated information from the methods, please reword.

Repeated details have been removed
“To examine the sensitivity of simulated HIWC to changes in model grid spacing and microphysical parameterizations, we performed a set of simulations configured with four nested domains and three microphysics schemes.”

Line 229: please reword “Below this line, ice particles are associated with weak updrafts and represent mostly in-situ cirrus clouds that are likely heterogeneously, or, less likely, homogeneously.”. The latter half of the statement should read something like, “…in-situ cirrus clouds that likely originate from heterogeneous nucleation, and less likely form homogeneous nucleation.”

Corrected as suggested

Line 246: this is not clear based on Figure 12

Corrected to (Fig.9a, left).

Line 266: again, the 99th percentile should be defined somewhere in the text, in line with my previous comment. The figure does not show the 99th percentile of the data for the measurements or any of the simulations except for the Thompson scheme. I would also say that all simulations overestimate smaller ice crystals compared to the observations. See comments on Figure 15 below. Finally, why does the Thompson scheme stand out from the others between 10-15 um.

The 99th percentile is defined is line 204

“The 99th percentile is used to present HIWC which refers the top 1% of simulated or observed IWC values.”

The 99th percentile (top 1%) is calculated for all schemes but not shown in figure 15.
Indeed, all simulation overestimate smaller ice crystals compared to the observation.

The following sentence has been added in lines 305-307: “…, especially for Thompson. It can be explained by the slightly warmer CPT (Fig. 12) compared to the other two schemes which can limit ice crystal growth (Fig. 13) and LWC (Figs. 7 and 8).”

Line 315: please add a reference to figure 14 after “due to the difference in RHi”

Old figure 14 became figure 12.

reference to figure 12 has been added.

Line 391 (319): isn't the statement "the low rate of in-situ nucleation" contradictory of the statement above saying that the PSD indicates more in-situ nucleation of planar ice crystals in the anvil?

There is no contradiction: The planar habit experiences higher in-situ nucleation but strong decrease in ice crystals with large maximum dimension due to sedimentation.
The lower rate of in-situ nucleation is for columnar habit. For clarification, the sentence is reworded as follows in lines 353:354

"For columnar habit, the low in-situ nucleation rate prevents the ice crystals from reaching the sedimentation threshold and from undergoing a strong decrease in the number of large ice crystals encountered by the planar habit (Fig. 15)."

Line 330: shouldn't "scatter plots of RHi as a function of fall speed" be "fall speed as a function of RHi"?
Corrected as suggested.

Also, it is unclear why this connection is being made. These are seemingly unrelated quantities. Can the authors add some explanation perhaps?

The RHi regulates the nucleation and growth rates of ice particles. The shape of ice crystals can change significantly over a few degrees Celsius (e.g., Magono and Lee 1966; Bailey and Hallett 2009) and influences its fall speed in the cloud (Heymsfield and Iaquinta 2000).
Freshly nucleated ice crystals of small sizes and low fall speeds can be either in situ or of liquid origin. The figure indicates a wide range of RHi for the same fall speed, especially for slower falling ice particles. For small ice particles (blue color) and low fall speed (<20 cm s^{-1}), the associated RHi values help distinguish in-situ and homogeneous nucleation with high RHi from liquid origin with lower RHi.
Moreover, this figure reveals that planar habit dominates in-situ nucleation, especially in the anvil.
Therefore, this figure relates these three variables to help identify the origin of the nucleation process, its region in the cloud, and the associated habit.

Heymsfield, A. J., and J. Iaquinta, 2000: Cirrus crystal terminal velocity. *J. Atmos. Sci.*, **57 ,** 916–938

Line 390: why does adding more realistic and complex ice habits alter the radiative heating of clouds? Please elaborate in the text to make it clear for the reader.

The following details have been added to the manuscript:
"Radiative properties of cirrus depend on the optical properties of its ice crystals, which in turn depends on the microphysical properties such as particle shape, particle size distribution, ice water content (eg., Key et al., 2002) and number concentration (eg, Liu et al., 2003). Previous studies (eg. *Takano and Liou* 1989) found that single-scattering albedo is related to aspect ratio and absorption coefficient of ice crystals. Also, Fu (1996) calculated the single-scattering properties of columnar and planar habits, using the geometric-optics-integration-equation technique [*Yang and Liou*, 1996b]. Key et al (2002) quantified significant sensitivity of ice cloud radiative properties to ice crystal optical properties of seven ice particle shapes.

The current study reveals a significant change in these microphysical properties across ice habit (Fig 9, Fig 11, and Fig 14). Additionally, ice crystal capacitance is parameterized differently from one ice habit to another depending on the maximum diameter of the crystals. This affects the growth rate of ice crystals differently, which in turn affects microphysical processes to alter the size distribution of ice crystals (Fig. 14), further modifying radiative heating."

Fu, Q.: An accurate parameterization of the solar radiative properties of cirrus clouds for climate models, J. Climate, 9, 2058– 2082, 1996.

Key, J. R., Yang, P., Baum, B. A., and Nasiri, S. L.: Parameterization of shortwave ice cloud optical properties for various particle habits, J. Geophys. Res., 107, doi:10.1029/2001JD000742, 2002.

Liu, H. L., Wang, P. K., and Schlesinger, R. E.: A numerical study of cirrus clouds. Part I: Model description, J. Atmos. Sci., 60, 1075–1084, 2003.

Takano, Y. and Liou, K. N.: Solar radiative transfer in cirrus clouds. I. Single-scattering and optical properties of hexagonal ice crystals, J. Atmos. Sci., 46, 3–19, 1989.

Yang, P. and Liou, K. N.: Geometric-optics – integral-equation method for light scattering by nonspherical ice crystals, Appl. Optics, 35, 6568–6584, 1996.

Line 419: "As the properties of cirrus clouds may differ depending on the geographical location and proximity to convections, this study is associated with a deep convection case during Asian summer monsoon." Please reword this statement as the first clause does not motivate the second. Also, add an article before "Asian summer monsoon".

Reworded as follows in line 466:
"The properties of cirrus clouds may differ depending on geographical location and proximity to convection. This study focuses on a specific case study associated with deep convection during the Asian summer monsoon."

Line 427: I don't think the sensitivity to grid spacing was really that clear from the figures.
Really, what is more important is the difference between the three schemes. This is in line with my comments of Figure 7, 8, and 9 below.

We agree that is not always significantly sensitive to change in grid spacing, except for Ishmael scheme, especially from 3k to 0.3km. We reworded the sentence to highlight is restriction as follows in lines 473-476

"Comparison of simulated microphysical properties to Stratoclim measurements shows that simulated HIWC in the TTL is significantly sensitive to horizontal grid spacing only with Ishmael scheme, with HIWC increasing substantially as grid spacing decreases from 3 to 0.3 km."

Line 433: change “vertical grid spacing” to “vertical resolution” if you want to keep this sentence. Otherwise, it reads as random and out of place relative to the rest of the paragraph. Consider placing it somewhere else.

Changed as suggested

Figures

General comment on all figures: those with subplots should include a label (a, b, c…) to help distinguish them within the main text. In some figures you refer to the Jensen scheme and other you label it as Ishmael. Please use a consistent label. Moreover, please chose a different colormap than Jet where applicable. This is not colorblind friendly, it introduces bias in interpretation of the results, and in some cases the reader cannot distinguish between the modelled data and the observations as they are plotted in the same color (e.g., Figure 10).

Corrected as suggested: All figures with subplot include labelings (a,b,c,…)

Corrected: Jensen → Ishmael

Corrected: All figures are tested for color blindness and colormap adjusted accordingly.

Old figure 13 -> new figure 11 , for the distribution of N_ice for each habit color of aggregates distribution and 99th and max values are changed to be color-blind friendly. And distinguish between habits.

Figure 1: this is a great figure. Though, please describe everything in the figure in the caption. For example, next to the arrows are the altitude of the aircraft and the distance, which I assume is the distance from the origin, but I can’t tell for sure.

The following details are added to the caption of Figure 1:
“The corresponding altitude and distance from origin are indicted near each arrow (upper)”

Figure 4: this is a confusing figure as it looks very much like a tephigram. The caption should be much more descriptive, stating something along the lines of vertical resolution as a function of model vertical level. Also include a description of the shaded area. Also, do not refer to it as “vertical spatial resolution” as spatial indicates a single level and may confuse readers.

Corrected as suggested:
The new caption: “Figure 4: Vertical resolution as a function of model vertical level. The blue shaded area represents the TTL”

Figure7, 8, and 9: I am not these three figures and the associated discussion really portray the message the authors would like to convey, which is to show sensitivity to model domain. I am not sure this is really needed for this study. I would rather show the differences in the inner domain between the three microphysics schemes in one figure and move these three figures as they are now to an Appendix.

As the reviewer suggested, Figure7, 8, and 9 are shifted to Appendix and new Figure 7 is created with only profiles from the domain D04. (a) for Ishmael scheme, (b) for P3 scheme and (c) for Thompson scheme.

Figure 14: please move the legend below and make it larger so it is easier to read.

Modified as suggested. Also, additional description has been added to include the level of cold point temperature.

Figure 15: I would show the full range to be in line with the discussion on Line 266 and then perhaps place a box over the region of interest (0-50 um) and shown a blow up of that range as a subplot.

To be in line with the discussion, we added the 99^{th} values on the figure 13 itself.

Figure 19: can the authors add fit lines to each subplot to aid their discussion in the paragraph starting on line 357?

As suggested: Fit lines with corresponding equations and correlation coefficients have been added to each subplot of figure 17 (old figure 19).

Response to Minor comments:

Sometimes you are missing an article is cases where you need one. For example, on Line 442 “and Ishmael scheme…” should read “and the Ishmael scheme…”. Please review all instances of this.

Corrected throughout the manuscript as suggested.

Line 418 change “fewer” to “few”

Changed as suggested

Line 419 change “significantly” to “frequently”

Changed as suggested

Line 373: delete “the” before “controlling”

Changed as suggested

Line 458: make hydration lowercase.

Changed as suggested